

OUR BOOK SHELF

Researches on the Glacial Period. By P. Kropotkin. First fascicule. 827 pages in 8vo. With Maps and Woodcuts in a separate brochure. (*Memoirs of the Russian Geographical Society*, vol. vii., 1876.)

THE book consists of two parts. The first is a detailed account of a journey in Finland and a short visit to Sweden, both made in 1871 under the auspices of the Russian Geographical Society, for the special purpose of studying the glacial formations and the *ösar* (eskers or kames). The second part is an inquiry into the meaning and value of various evidences of the glacial period—the striation of rocks, the forms of rocks and mountains, the boulders, the loose deposits, and the moraines and *ösar*. Out of the seven chapters into which this part is divided only the three first (sketch of the development of the glacial theory, striation, and forms of mountains) appear in this fascicule, and the two last (loose deposits and their classification, moraines, and *ösar*) are summarised at length in an Appendix.

The first fascicule is illustrated by a hypsometric map of Finland (southern half) with all known *ösar* shown upon it; by a map of the most interesting, esker Pungaharju, five miles long; by some other maps and sections of less importance; by a section on a large scale of the loose deposits along the Tavastehus-Helsingfors Railway, and by ninety woodcuts, a large part of which are sections of *ösar*.

The main conclusions as to the glaciation of Finland are in accordance with those arrived at by Messrs. Erdmann, Wiik, Helmersen, and Schmidt, viz., that this low table-land, continuous along its north-western and southern borders with two low and flat border-ridges, was covered with an immense ice-sheet which, creeping from Scandinavia, crossed the Gulf of Bothnia, traversed Southern Finland in a direction south by east, crossed the Gulf of Finland and crept further on in the Baltic provinces. The numberless striae, the positions and directions of which exclude any suspicion of their having been traced by floating ice, the striation on the islands of the shallow gulfs, together with that of the Onega basin, the Neva valley, and the Baltic provinces, the uninterrupted sheet of till, *i.e.*, of a true unstratified and unwashed morainic deposit covering Finland, the numberless moraines parallel to the glacial striae, and hundreds of other evidences, settle the existence of such an ice-sheet beyond any doubt. As to traces of marine formations, there are none above a level of about 100 to 120 feet; only local lacustrine deposits cover the till above this level.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indian Rainfall and Sun-spots

I HAVE observed no notice in NATURE¹ of an important discussion which took place a month ago at one of the Royal Society's meetings on Dr. W. W. Hunter's report on the cycle of rainfall in India, and its coincidence with the periods of eleven years disclosed by sun-spot observations. As one interested in solar research I have carefully considered that report, and I think the author has made out a case within the limits which he assigns to himself. The application of the mathematical law of errors has not altered this opinion in my mind, and from a consideration of the whole subject I have been led to the following conclusions:—In the first place I would remark that in certain

¹ See abstract of Gen. Strachey's paper on another page.

meteorological elements, of which the rainfall throughout the world is probably one, and the barometer in these latitudes is another, oscillations which we regard as non-periodic, are very large compared with periodic variations. The consequence will be that in dealing with a series of barometric observations in these latitudes, the mean difference of individual observations from the mean of the whole series, or in other words, the mean irregularity, will not be materially modified by the introduction of the comparatively small semi-diurnal variation. But this is no argument against the existence of such a variation, nor is the fact that at Madras the mean rainfall irregularity is not greatly reduced by the introduction of an eleven-yearly cycle any argument against the existence of such a cycle. As a matter of fact, this mean irregularity is reduced, although perhaps not very markedly, by the introduction of this cycle. The true test of a physical cycle is its repetition, and, since in the present important aspect of this question we cannot, perhaps, calmly wait for other sixty-four years' observations before venturing a conclusion, let us now endeavour to break these sixty-four years up into periods, and see whether we obtain any traces of physical persistence from this method. Grouping, as Dr. Hunter has done, the sixty-four years' Madras rainfall into series of eleven years, beginning with the first in 1813, we obtain the following table:—

Years employed.	Year of Series.										
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
A. 1813-23 ...	45'11	32'41	56'00	41'16	63'56	76'25	36'33	70'01	47'13	59'61	26'62
B. 1824-34 ...	33'72	56'05	60'73	88'41	37'89	36'87	32'43	44'35	18'45	37'11	39'00
C. 1835-45 ...	41'47	44'76	49'26	32'33	53'07	58'65	38'32	36'18	50'28	05'36	38'05
D. 1846-56 ...	79'81	80'99	54'76	39'81	36'88	64'32	72'69	35'82	43'20	32'32	46'99
E. 1857-67 ...	52'95	48'50	55'14	27'64	37'19	38'18	54'61	47'23	41'64	51'39	24'37
F. 1868, end.	41'43	32'31	74'10	56'35	73'67	51'83	62'90	37'12	21'49		
Whole period.	49'1	49'2	58'3	50'9	50'4	54'4	52'9	45'2	37'0	49'2	35'0

In this table 3, 4, 5, 6, 7 embrace the maximum rainfall group, and 8, 9, 10, 11 the minimum rainfall group, and the sun-spot maximum occurs generally about the beginning of 3, and the sun-spot minimum a little before 11.

We have, therefore, taking the means of the five maximum rainfall years a result = 53'4 for the whole six series, and also, taking the means of the four minimum rainfall years, a result of 41'6 for the whole six series.

But we can obtain similar results for each individual series as under:—

	Max. Group.	Min. Group.
Series A ...	54'7	50'8
„ B ...	51'3	34'7
„ C ...	54'3	47'5
„ D ...	53'7	39'6
„ E ...	42'6	41'2
„ F ...	63'8	29'3 (incomplete.)

We have thus considerable evidence of repetition. In connection with this it will be interesting to see if there is any other physical difference indicated between years of maximum and minimum spots besides mere difference of rainfall. Now a very interesting additional peculiarity has been indicated by General Strachey, who has observed that the conception of a cycle of eleven years introduces a decidedly diminished mean cyclical deviation for the minimum period. General Strachey has, no doubt, likewise remarked that this is not chiefly due to those particular years that are nearest the sun-spot minimum. I do not, however, see that we have any right in tracing a connection between solar epochs and rainfall values to insist that the minimum of the one shall correspond absolutely with the minimum of the other, and the maximum of the one with the maximum of the other. In conclusion, the fact that the introduction of a solar cycle diminishes considerably the deviation for minimum years is one of very great interest, since it is these very years that have become so practically important. I trust, therefore, that further attention will be devoted to this very interesting inquiry.

BALFOUR STEWART

Natural History Museums

I AM sure that many readers of NATURE will heartily thank Prof. Boyd Dawkins for his valuable articles just published in

your journal on the need of establishing natural history museums in the principal towns of our country. The ideas set forth cannot fail to be reciprocated by a largely increasing number of students who, like myself, are suffering under the disadvantages of not having local museums for reference and in which to compare specimens and examine the various natural history objects which I wish to study. In addition to a museum, I think such buildings should contain lecture-rooms specially fitted up for scientific lectures, as the value of able discourses is frequently lost for want of clearness in illustration.

The professor cannot over-estimate the value of museums, as every lover of natural history cannot be a collector; but every one in full possession of his faculties can observe so far as he has the power of seeing, and if he cannot examine the wide field of nature for facts he will at least examine the proofs of them in the museums, if at hand.

A few personal observations may serve to show the difficulties under which the so-called working classes have to labour in the pursuit of knowledge.

Some years ago I began to study the works of Sir C. Lyell and other authors on geology, and while so engaged I many times travelled eighteen miles after a hard day's work to compare specimens in the old museum, St. Peter's Street, Manchester. I had tabular views of the characteristic British fossils at hand, but as perfect specimens only are figured, I experienced a doubt and uncertainty pretty nearly in everything I wanted to compare, while in the museum I could find the actual specimen sought after with which to correlate those of my own. The flash of satisfaction experienced by a collector on comparing his objects with those in a well-arranged museum is indeed very great, and there are few things more likely to stir him up to renewed efforts. But the interest of museums is not confined to the collectors of natural history objects; it extends to every man who reads and cares to master the objects about which he reads. In this way his knowledge of things becomes real and he expresses himself with confidence, and in many cases has decided while others are thinking. To show further the need of museums I may state a fact perhaps not generally known, that in one place in the north of England a large number of science students have formed themselves into an itinerant society moving from place to place to suit the convenience of the various members who reside apart. The meetings are generally held at a respectable inn on Sunday evenings, at which papers are read by the more ambitious members, and any interesting objects named, which some of the party never fail to bring up, and their habitat declared.

If the corporate bodies or the educational department of the State would only undertake to provide museums in the principal towns of our country I feel sure that the cry of continental superiority would soon vanish. At home we have the materials out of which the philosopher and the artisan can spin the fibre of future greatness by rightly directing the forces of nature, but the isolated fragments want collecting and receptacles providing in which to store them. Many lives like that of the Banff naturalist could be written if only known, and Prof. Dawkins could not have fixed on a centre of operation more favourable from which to begin than that of Oldham. Men more selfishly removed above praise, working for science for its own sake, he cannot find, and it is a pity that they have not a common repository in which to store their invaluable collections beyond their own full cabinets. I hope the professor's articles will be a means of calling attention to the desirability of establishing museums for the better diffusion of scientific knowledge.

I write from the point of view obtained by my own experience as a working man who has done his best to educate himself.

WM. WATTS

Corporation Waterworks, Oldham, June 16

Koenig's Tuning Forks

ON vient d'attaquer en Angleterre l'exactitude du diapason officiel français. Mr. Alexander J. Ellis ayant trouvé que les notes d'un tonomètre, composé de 65 anches d'harmonium et construit par Mr. Appunn, ne s'accordaient pas avec ce diapason, a cru devoir déclarer dans un mémoire publié par le *Journal of the Society of Arts* (25 Mai, 1877), et dans votre journal (31 Mai, 1877), que le La₃ normal français donnait non pas 870 vibrations simples, comme on l'avait cru jusqu'à présent, mais bien 878 vibrations simples.

Mr. Ellis ayant constaté de plus que les diapasons de ma construction s'accordaient parfaitement avec le La₃ français, n'a pas

hésité à affirmer que tous ces diapasons, y compris ceux de mon grand tonomètre, qu'il n'a probablement jamais vus, et en tout cas jamais pu examiner, étaient nécessairement inexactes. N'ayant pas à ma disposition l'instrument dont s'est servi Mr. Ellis, j'avoue que je ne serais trouvé assez embarrassé pour dire immédiatement, par où pêche cet instrument au point d'avoir donné entre les mains de Mr. Ellis des résultats si extraordinaires; heureusement je me suis rappelé une lettre de M. Helmholtz à Mr. Appunn et publiée par ce dernier lui-même dans une brochure sur les théories acoustiques de M. Helmholtz; cette lettre concerne justement un instrument de même nature du même constructeur et explique suffisamment les surprenantes découvertes de Mr. Ellis. "J'ai examiné à plusieurs reprises votre tonomètre," écrit M. Helmholtz à Mr. Appunn, "et je suis étonné de la constance de ses indications. Je n'aurais pas cru que les anches pussent donner des sons aussi constants que ceux que donne l'appareil, grâce à votre méthode pour régler le vent. L'instrument varie un peu, il est vrai, avec la température, comme seraient aussi des diapasons; on ne peut donc s'en servir pour la détermination des nombres absolus de vibrations que lorsqu'on peut travailler dans une pièce qui n'est pas chauffée par un poêle. J'ai compté les battements à l'aide d'un chronomètre astronomique, et je crois que votre pendule à secondes a été légèrement inexact, car, si les nombres de battements s'accordent très bien entre eux, le nombre absolu en a été non pas de 240, mais de 237 à la minute. La température, qui était assez basse pendant mes expériences, a pu y être pour quelque chose, mais on peut éliminer cette influence en comptant jusqu'au bout les battements d'une tierce majeure, ce qui m'a pris un quart d'heure. J'ai trouvé ainsi pour mon diapason de Paris 435'01

vibrations, ce qui l'accorde à $\frac{1}{40,000}$ pris avec le nombre officiel de 435'00 vibrations."

Cette lettre prouve que le nombre entier des battements de l'octave du tonomètre essayé par M. Helmholtz était de 237'64 = 252'8, et sa note fondamentale de 505'6 vibrations

60 simples au lieu de 512 vibrations simples. En comparant cette note de 505'6 vibrations simples avec un diapason donnant réellement 512 vibrations simples, Mr. Ellis eût trouvé ce dernier de 6'4 vibrations simples plus aigu, et l'eût sans doute considéré comme donnant 518'4 vibrations simples. Or il a trouvé 516'7 seulement pour mes diapasons de 512 vibrations simples avec le tonomètre dont il s'est servi; on voit donc que la note fondamentale de ce dernier était déjà plus exacte que celle du tonomètre examiné par M. Helmholtz puisqu'elle donnait 507'3 vibrations simples mais qu'elle restait encore assez loin de la véritable valeur.

Le fait que M. Helmholtz a pu trouver le nombre de vibrations exact du diapason officiel français avec un instrument de cette nature (et même encore moins parfait que celui dont s'est servi Mr. Ellis), en déterminant d'abord la correction de cet instrument, montre à l'évidence que Mr. Ellis a négligé de déterminer la correction du sien; il s'est donc beaucoup trop hâté de déclarer que ces petits tonomètres à anches d'harmonium sont les plus parfaits et les plus exacts qui existent, et de contester si légèrement les résultats obtenus par les Lissajous, les Despretz, les Helmholtz, les Mayer, etc., etc. RUDOLPH KOENIG

Paris, le 5 Juin

Antiquity of Man

MR. SKERTCHLY is absolute that I am mistaken; to me it appears that he has missed the point of my letter, and misinterpreted my views. His important discoveries of flint implements in early glacial beds are, I think, strongly corroborative of the opinions I expressed in my paper on the "Drift of Devon and Cornwall" (*Quar. Journ. Geol. Soc.*, vol. xxii. p. 88), and in that on the "Geological Age of the Deposits containing Flint Implements at Hoxne" (*Quar. Journ. Science*, July, 1876); but I willingly admit that in the present stage of the inquiry Mr. James Geikie has as much right to claim that they support his theory, and I agree with the latter that it is premature to discuss the relation of man to the glacial period, before we have settled what was the succession of events that occurred at that time.

Mr. Geikie contends that there were two or more glacial periods with inter-glacial warm or mild ones; I, that there was